



Essays in Nineteenth Century Economic History: The Old Northwest by David C. Klingaman;
Richard K. Vedder

Review by: Harry N. Scheiber

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nology. Both would probably agree that, as Morison put it, in the pre-Civil War period science offered few "general ideas or theoretical considerations that would help in building and making new things" (p. 90). While Morison is concerned with the provision of scientific theoretical underpinnings for technology in subsequent decades, Sinclair emphasizes the ways in which science's experimental method, intellectual rigor, and emphasis on original research had begun to influence Philadelphia industrialists before 1860. This is a topic that deserves much more research, but both books represent a significant contribution to the topic and to the general history of American technology.

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MERTON L. DILLON. *The Abolitionists: The Growth of a Dissenting Minority*. (Minorities of American History.) DeKalb: Northern Illinois University Press. 1974. Pp. xiii, 298.

Like oil wells, historical topics sometimes run dry. In this judicious study Merton Dillon, author of two fine antislavery biographies and several classic articles in the field, surveys the abolitionist experience from the American Revolution through the Civil War. Nevertheless, the procedure is a bit arid. Gilbert Barnes, Dwight Dumond, Louis Filler, Gerald Sorin, and others have traversed the same terrain with varying degrees of success. Dillon's contribution is no less worthy than theirs, and his mixture of sympathy and critical judgment provides a better balance than most other examples of the genre. Yet the book comes late in the day.

Dillon graciously acknowledges the work of recent scholars—Kraditor, Stewart, Thomas, Perry, the Peases, which he skillfully weaves into his organization; unfortunately, though, he does not challenge their sometimes contradictory views or move the topic into new directions. By and large, this work is a conventional narrative about the rise and impact of antislavery, chiefly in its political rather than intellectual phases. Given the author's undoubted knowledge of the complexities involved in abolitionist reform, the result is bland and homogenized. Not until the final pages does he raise a suggestive question: just how important were the abolitionists in the overthrow of slavery? Less than we might think, he replies, because, as he remarks earlier, "something beyond moral commitment and skill was needed to assure abolitionist success" (p. 83). Southern foolhardiness and volatility did more to stimulate anti-Southern reactions than

the actions of reformers alone. It may be an old perception, but he could have pursued it with greater vigor and precision than his predecessors did. Otherwise, Dillon hugs pretty close to familiar pathways. One would have liked to see less about Garrison and Tappan, more about antislavery lawyers who ingeniously forced the legal system to confront constitutional questions in the fugitive and personal liberty cases; less about the 1840 division and more about the sectional church schisms—in the mode of Donald Mathews; less about reform leaders in general, more about followers, about whom we still know little. As a straightforward, narrative survey, the work is certainly satisfactory, but as a provocative interpretation of antislavery significance, it is a mild disappointment.

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DAVID C. KLINGAMAN and RICHARD K. VEDDER, editors. *Essays in Nineteenth-Century Economic History: The Old Northwest*. Athens, Ohio: Ohio University Press. 1975. Pp. xiv, 356. \$12.00.

Viewed from a charitable perspective, this collection of eleven essays combines the talents of five seasoned scholars in economic history with the work of some newer contributors. This is a book of highly uneven quality, but there are some gems to be found in it. And it is produced handsomely. But viewed from another angle, it is astonishing that its publication can be justified at a time when university presses are constantly turning down excellent manuscripts of young scholars because of fiscal constraints. The longest of the contributions—an eighty-seven page essay on railroads and economic development from 1870 to 1890 by Jeffrey G. Williamson—consists of verbatim sections of Williamson's book, *Late Nineteenth-Century American Development* (1974). Another essay is merely a recapitulation of Roger Ransom's well-known published articles on social returns from canal investment. And I think two of the remaining chapters could never have passed muster with referees of even a middling-quality historical or economics journal.

What, then, of the other half of the book? The most valuable contributions here, apart from Williamson's, are by Robert E. Gallman and Richard A. Easterlin. It speaks poorly for the precision of the book's title that neither of these two essays treats principally the Old Northwest; both deal with agricultural output, income, labor force, and productivity for the nation as a whole in the nineteenth century. As one would expect of his work, Gallman's study reveals ingenuity and remarkable

industry in the gathering and analysis of data, together with rigorous presentation of quantitative measures from the prestatistical era. Gallman reconsiders earlier estimates (by Paul David and others), and he argues persuasively that agricultural productivity accelerated greatly after 1850. He offers a startling partial explanation: there was under-utilization of farm labor in the early nineteenth century. The hours of farm labor, he maintains, were possibly one-third less than the hours of contemporary manufacturing workers; there were many slack periods and "substantial leisure."

Easterlin's study provides invaluable estimates of output, labor force, and income of farms by state for 1840 and by region for 1840–1860. He addresses the intriguing problem of "start-up" low-income periods in the early development of what later became rich farming regions. Also considered is the question of productivity on Southern farms and plantations; Easterlin argues that the South did not compare unfavorably with other regions. Finally, he contends that productivity gains attributable to shifts to more fertile western lands were probably less important, at ca. 5.6 percent from 1800 to 1840, than other sources, which totaled about 30 percent.

David Klingaman deals with changes in wealth in three northeastern Ohio counties, in an essay that, despite its severely localized data base, links nicely with one by Lee Soltow on the growth of wealth in Ohio. The latter study leans heavily on the weak reed of tax valuation data from the early nineteenth century. An interesting essay by Don R. Leet argues that the decline in birth rates preceded industrialization and urbanization in early Ohio, a pattern largely explained by rising population-to-land ratios and consequent declining opportunity. But lacking in Leet's analysis is any consideration of alternative opportunities to life and work on local farms, or their effects on fertility rates.

Richard Vedder and Lowell Gallaway contribute a workmanlike statistical study of migration and the Old Northwest, long on interesting data but short on analysis. The other essays—on Old Northwest regionalism, on banking, and on land speculation—offer little that is novel to students of these subjects. Presumably Williamson's study will be evaluated, as it should be, in reviews of his own book—undoubtedly one of the most challenging works in the literature of American economic history, but hardly one that required partial reprinting in this format.

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WARD E. Y. ELLIOTT. *The Rise of Guardian Democracy: The Supreme Court's Role in Voting Rights Disputes, 1845–1969*. (Harvard Political Studies. Published under the direction of the Department of Government in Harvard University.) Cambridge, Mass.: Harvard University Press. 1974. Pp. xiv, 391. \$15.00.

ROBERT M. COVER. *Justice Accused: Antislavery and the Judicial Process*. New Haven: Yale University Press. 1975. Pp. xii, 322. \$15.00.

Whether inspiring or exacerbating, high-court decisions between 1954 and 1974 helped to shape the period. In turn, campus disagreements about public policies shape scholars' views of the more distant past. Most historians try to minimize this presentism. The authors of both books under review approach their subjects in candidly present-minded ways.

Ward E. Y. Elliott, a political scientist, has grappled with aspects of the history of Supreme Court decisions involving voting rights. It is unfortunate that he expended his large talents to prepare a book-length missile against many of his co-disciplinarians.

His argument appears to be that academic theorists, including some in judicial robes, were—and are—ill-equipped to solve democracy's problems. Elliott deplores the Court judgments favoring swift social changes rather than acceptable progress. He charges that the Court assumed a guardian responsibility which it could not sustain and which proved to be seriously out of phase with public opinion and with minorities' true needs. Elliott wants judges to be restrained priests, not crusading prophets.

Whatever the merits of Elliott's dour view of the Kennedy-Johnson-Nixon years, the *Rise* portions of his *Guardian Democracy* lessen the book's impact. Elliott's historical data fail to support his judgment that alternative policy institutions were ready, willing, and able to act if courts and judges did not, or that inaction was preferable to judicial intervention. As examples, Conron's and Gettleman's work on the Dorr Rebellion and *Luther v. Borden* raise questions concerning legislative sensitivity to which Elliott does not address himself. His Civil War and Reconstruction analysis depends on publications no more recent than the very early 1960s. A fair share of the rich newer work directly disagrees with Elliott.

History offers sharp alternatives to the author's convictions that only the representative branches of government should initiate or shape social changes. The Court's history suggests that the high bench should and can balance as well as check. And better balances were what the voting rights cases were about.